

Systems Thinking: the Explanation of Continuous Change

simulations reaches a steady state within the time span considered, until the population is artificially and externally held constant (ibid. 160).

The aim of the present work is indeed to arrive at (approximately) steady states, but these should arise from the interactions of the variables rather than be imposed quite arbitrarily from the outside.

[1979]

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THE SIMULATOR AS DEMIURGE

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Demiurge: (Greek *demios*, for the people, *ergos*, work): 1a, a Platonic subordinate deity who fashions the sensible world in the light of eternal ideas; 1b, a Gnostic subordinate deity who is the creator of the material world; 2, something that is an autonomous creative force or decisive power.

Chambers Twentieth Century Dictionary

Systems thinking has increasingly been regarded as an essential component of archaeological theory since its introduction to the subject nearly two decades ago (Binford 1962). Yet despite the undoubted attractions of simulating the past, usually within some appropriate systems framework, simulations in this field have so far been rather few in number (Thomas 1972, Cordell 1975 and Zubrow 1975 being among the first). Only very recently has system dynamics modelling been applied to past cultural systems (Hosler, Sabloff and Runge 1977; Shantzis and Behrens 1973).

It is the very essence of simulation modelling that the simulator creates a device which will produce some representation of reality. A good simulation mimics reality closely. But a successful simulation is rarely achieved at the first attempt. If at first you don't succeed you try again, modifying or tuning the device to give a better - that is, closer to real - output. The simulator proceeds in some senses very much like an inventor, trying to get his infernal device to work. The theoretical content of his creation is often not altogether clear, being sometimes implicit in the structure of his invention, rather than set out in advance, neatly and explicitly, in the form of a number of theoretical relationships.

These two aspects - the frequent necessity for successive attempts before it 'works,' and the deliberate anticipation of 'unexpected' behaviour - often combine to make systems modelling seem a very

creative process, in which the new creation has behaviour, indeed, almost a life, of its own. It is this aspect, which often makes systems modelling seem such an exciting undertaking, that prompts the title to this chapter.

Some time ago I pointed out that 'the logical structure of a system model . . . differs fundamentally from that of a hypothetico-deductive explanation' (Renfrew 1973, 1929), and, as several commentators have stressed (Flannery 1973, 51; Perlman 1977, 321), it is important not to become caught up too readily in any preconceived notion as to what logical form a scientific explanation *must* take. Recent excessive emphasis on one specific explanatory mode, the Hempel-Oppenheim covering law model, has risked elevating to dogma what should be no more than informative precept. As Binford (1977, 6) states of theory building: 'This is a creative process for which there are no methodological rules to ensure success.'

Bell (1981) has rightly emphasized the importance of being aware of the procedures by which our models may be compared with reality, thus allowing them to be verified or refuted. Such procedures are the hallmark of empirical science, providing the criterion which distinguishes it from mere metaphysical speculation. Yet problems remain here which still await clarification. That is, indeed, one of the fascinations of this field, which is clearly at a very creative stage of development.

Systems and Systems Thinking

The systemic approach has been widely advocated in archaeology (Wood and Matson 1973; Munton 1973; Hill 1977a), and a number of optimistic claims have been made, many with good justification. But there have been evident confusions so that terms like Systems Theory, General Systems Theory, System Dynamics, and Systems Thinking are employed almost interchangeably. That perhaps in part excuses the almost entirely negative critique, 'What can systems theory do for archaeology?' recently contributed by a philosopher (Salmon 1978), who does not make these relevant distinctions. In discussing the systemic approach to archaeology, it may be useful to try to distinguish between

1) *Systems Thinking*: A general philosophical approach to the subject, an *intuition framework* within which specific problems may be tackled;

2) *General Systems Theory*: A consideration, initiated by von Bertalanffy, of the various properties and behaviours shared by a wide range of superficially very different systems;

3) *Mathematical Systems Theory*: An explicit body of mathematical theory which (in common with System Dynamics Modelling) may be regarded as a special aspect or branch of the Theory of Dynamics Systems (see 5 below). It entails the mathematical theory of feedback control systems and optimal control, much used by engineers, but so far has little direct relevance to archaeological problems.

4) *System Dynamics Modelling*: The approach pioneered by Forrester and his colleagues of expressing the behaviour of a complex system by a large number of equations of state and of using the computer to calculate the successive values through time of all relevant parameters by means of these equations, hence achieving a simulation; and the

5) *Theory of Dynamical Systems*: The broad undertaking of mathematicians to create a theory for all systems that evolve according to a dynamical law. These are generally described either by differential equations or by difference equations, whose solutions may be studied by qualitative as well as quantitative methods. Although there are as yet no explicit archaeological formulations using this framework, the methods are of such power and relevance that significant progress is to be anticipated.

Let it be at once acknowledged, as Salmon (1978) has rightly stressed, that there is no single body of explicit formulations called Systems Theory which is ready and available for application to elucidate the behaviour of humans or cultures. On the other hand, there is already, at this early stage, a good deal of useful experience available.

Systems Thinking constitutes in my view the most important advance in archaeology in recent years. It implies the use of series of concepts that are now becoming familiar and, one may hope, through that familiarity, are emerging as something more than mere jargon expressions. Among these important notions are: closed system, open system, subsystem, input, output, trajectory, negative feedback, positive feedback, homeostasis, stable and unstable equilibrium, steady state, state variable, flow rate, morphogenesis, transformation, exogenous and endogenous inputs, variety, equifinality, and so forth.

The value of such an approach and such terms is simply that, for the first time, they offer a framework and a language for the analysis of complex entities, such as societies, organizations, or cultures, in a manner that allows a direct and simple treatment without denying the difficulties inherent in such a task.

The underlying ideas are obvious enough, like most good ideas, when one has acquired a familiarity with them. But to measure the gain, it is sufficient to make comparison with the level of explanation which preceded their introduction. In archaeology, this often amounted to little more than a bald narrative of a sequence of events, often given a false coherence in the light of a diffusionist interpretation. If anyone doubts the value of systems thinking, let him make comparison with almost any purported explanations in the archaeological literature offered prior to 1960 (although every archaeologist could compile his own short list of honourable exceptions).

Systemic ideas were introduced to archaeology by Binford (1962) and set out with great coherence by Clarke (1968); they have already inspired a number of studies (Flannery 1968, Renfrew 1972, Plog 1974). No well-organized body of theory is implied, although it is not necessarily excluded. Rather, systems thinking is simply an approach to the world within one intuitional framework, which implies the exclusion of others that are incompatible with it. Such a framework is, of course, implicit within both General Systems Theory and System Dynamics modelling and is common to simulation in general.

General Systems Theory, originated by von Bertalanffy (see von Bertalanffy 1950), is concerned with the formal correspondence of general principles in the behaviour of systems, irrespective of the kind of relations or forces between the components. As such, it goes far beyond the scope of the anthropologist or archaeologist, although patterns may indeed be observed in cultural systems which are common to those of other kinds. Exponential growth and logistic growth are well-known examples (Hamblin, Jacobsen, and Miller 1973).

A similar although more restricted intention underlies the work of a number of anthropologists who seek to make general statements about the behaviour of sociocultural systems (see Flannery 1972; Rathje 1975; Gall and Saxe 1977; Athens 1977; Cherry 1978; Renfrew 1979). The concern here is limited, of course, to systems composed of human societies, but with this limitation, the aim is, indeed, very general.

It would be a mistake to claim that very much has yet been achieved along this path but a still graver one to deny the importance of the undertaking, for the aim here is close to that to which Binford (1968a, 27) many years ago inspired: 'Our ultimate goal is the formulation of laws of cultural dynamics.' Today this need not

be read as implying that the formulation will be in a strictly Hempelian lawlike form, nor indeed in any anatomization of a system into specific subsystems. That the goal is as yet dimly perceived does not make it any the less real. To dismiss General Systems Theory on the grounds that it does not measure up to some *a priori* concept of 'theory' risks discarding the conceptual baby with the philosophical bath water.

Mathematical Systems Theory is a well-developed field, where the behaviour of precisely defined control systems is subjected to rigorous analysis. Like System Dynamics modelling, it may be seen as a special branch of the Theory of Dynamical Systems. Here the consideration centres on the input and output of a conversational device which lies between them – often a control mechanism. The distinction between Mathematical Systems Theory and the Theory of Dynamical Systems is lucidly expressed by Berlinski (1976, 110) in writing of the former:

The fundamental systems here are much like dynamical systems. The method by which engineering objects are classified, moreover, mirrors the organization of the theory of differential equations. But it would be a mistake to dismiss the differences between engineering and dynamical systems: engineering is preeminently a concern for the relationship between inputs and outputs, while the mathematical focus of the theory of dynamical systems is on the equations themselves and the models in which they are satisfied.

Usually feedback control is employed, and often optimization procedures to minimize total expenditure or energy, or to operate in least time. Such work can trace its origin back to the same interest in cybernetics (Wiener 1948, Ashby 1956) which is ancestral to much current systems thinking. But it appears to have little to offer archaeology at present, its explicitly formulated problems are not our problems, and few of ours can ever hope to be given definitive formal solutions in this way.

A comparable formulation has nonetheless been attempted in the study of political science by Easton (1965), and the same schema of the archaeologist's culture systems as a 'black box' was set out by Leach (1973, 66). The archaeological or anthropological cases are much less rigorously defined, however, than those of the control engineer, and for this reason are more profitably discussed within the more general framework of the Theory of Dynamical Systems.

Systems Dynamics Modelling involves a particular approach to the problems of dynamical systems devised by Forrester (1961,

1969, 1971). Dynamic behaviour simply means behaviour analysed with time as an important variable. There can be many kinds of dynamical systems models, all of them giving explicit formulations for the behaviour of a system through time. System Dynamics models represent a subset of this general field of dynamical systems models, whereby a real and rather complex system is modelled by a complicated structure rich in feedback loops. The components are accumulations or reservoirs, measured by a level (state) variable, between which there is activity measured by a rate (flow) variable. An equation is written for each component of the system stating (predicting) its level or rate at the next point in time in terms of the present levels or rates of all the other components. The computer uses these numerous equations to calculate the values for all these state and flow variables at the next point in time, given the initial values. Then it proceeds to do the same again for the next time point, and so, by a series of iterations, the values of all the variables of the system are simulated.

The model discussed by Zubrow (1981) is a good example, displaying the rather complicated 'spaghetti-and-meatballs' network of flows and interrelations common to such constructions. Despite their apparent complexity, they certainly produce clear results.

The construction of such a model does not necessarily imply any remarkably original theoretical insights. It can be simply a systematic and painstakingly explicit analysis of a familiar if rather complicated situation. But Forrester (1973) has effectively made the point that the behaviour of such complex systems is often counter-intuitive. The whole is greater than its parts. The value of the technique as a research tool lies precisely in this counter-intuitive behaviour.

The Theory of Dynamical Systems is a general approach to the problem of change that, in a sense, subsumes the more specific concerns of Mathematical Systems Theory and System Dynamics modelling as defined above but is not limited by the specific assumptions and concerns which serve to define them more closely. It entails the attempt, by specifying the influences causing short-term changes, to derive the long-term behaviour of the system, and involves the use of difference equations or differential equations.

The state of the system is the smallest collection of numbers that must be specified in order to predict uniquely the system's behaviour: the variables in question are related by means of a set of differential equations. The solutions of the equations allow, or would allow, the prediction of the future behaviour of the system.

But the solutions of many sets of differential equations cannot be found quantitatively, and it is the *qualitative* study of the differential solutions and their properties, interpreted geometrically, which often proves enlightening.

The approach, and its archaeological potential, has been lucidly set out by Cooke (1979), and considered by Berlinski (1976, 47–51), as a preliminary to his critique of System Dynamics modelling undertaken by Forrester and Meadows. The mathematical literature, which extends back a century to the pioneering work of Poincaré, is extensive (see Chillingworth 1976). Yet despite its technical difficulty for the nonmathematician, some aspects of this approach are accessible to archaeologists: the use of phase portraits to illustrate the geometrical properties of trajectories is clear and illuminating at a general level. In this way, one is brought to grips with such fundamental notions as equilibrium, stability, periodic and recurrent motions, attractors, bifurcation, and other important ideas to which the mathematics naturally gives rise.

Living systems, including culture systems, exhibit such modes of behaviour and properties as these, and the opportunities offered by Dynamical Systems Theory to examine such behaviour in the most abstract way possible is valuable. It leads naturally to the pre-occupations of mathematical biologists such as Rosen (1979) or Waddington (1977), with which archaeologists are discovering much in common. Catastrophe theory, for example, may be regarded as a branch of the Theory of Dynamic Systems. Important concepts arising from this approach, such as the notion of the chreod and of the epigenetic landscape (Waddington 1977, 103–14), will be found highly relevant to the long-term behaviour of human societies.

Archaeological applications have hardly yet been developed, but in the adjacent field of ecology, the elegant investigation of the predator-prey relationship through the Volterra-Lotka equations (Maynard-Smith 1974, 19–27) suggests how fruitful this general approach can be in practical cases. Equations of comparable form are now being used as models of interaction between two human groups rather than between two species (Cooke and Freedman n.d.), and other explorations using similar approaches are sure to follow.

It seems noteworthy that the pessimism of Berlinski (1976) concerning existing systems analysis in the social, political, and biological sciences does not appear to be directed either toward Dynamical Systems Theory or its application in ecology. My own

optimism is based, on the one hand, on the appropriateness of systems thinking for the expression of archaeological problems and, on the other, on the apparent suitability of Dynamical Systems Theory for investigating relationships expressed in systems terms. Nonetheless a gap remains to be bridged: either archaeologists must learn more mathematics (and the right kind of mathematics) or mathematicians must prove willing to immerse themselves in the theoretical preoccupations of modern archaeology.

From the above, it will be seen that the 'systemic approach' can mean a number of different things, and that these should be carefully distinguished. Of course, the term 'system' can be defined in a multitude of different ways, but its chief value in archaeology is in its applicability to the very elaborate complex of interactions which any sociocultural entity represents, which can always be subdivided into a number of subsystems, each meriting analysis in its own right. It is this very complexity which makes the systems approach, with its own flexibility and ability to cope with elaboration, so useful.

This may not be the occasion on which to take issue in detail with what I feel are the problems arising from Salmon's treatment of this general topic. One feature is the apparent equation of mathematical models in general with systems theory in particular. Mathematical Systems Theory is one very specific and perhaps rather minor branch of mathematics. To say of the whole general class of mathematical models used to describe the real world, as exemplified by the simple Gas Law $PV = RT$, 'the classification of such models is thus an important part of Mathematical Systems Theory' (Salmon 1978, 179), may introduce a confusion. The treatment of mathematical models in general is certainly provocative. 'Mathematical models, even when they fit, do not in and of themselves constitute satisfactory explanations' (1978, 182). My main objection to such a statement is not so much that it is questionable (as I believe) but that it has little specific bearing upon the role of systems theory as such in archaeology.

The Nature of Simulation

Simulations differ in procedure and purpose. Only a few of them fall within the class of System Dynamics models, including the example set out by Zubrow (1981) and the instance presented by Sabloff (Hosler, Sabloff and Runge 1977). Many simulations – particularly those of high complexity – are nonetheless approached within the intuitionist framework of systems thinking, but this is certainly not a precondition for a simulation study.

The whole field of simulation, in archaeology and beyond had been thoroughly reviewed by Aldenderfer (1981a), the first such comprehensive survey, and only a few comments are required here.

First, it may be useful to distinguish between two classes of simulation, although many examples fall within both. In the first class, the *output* alone represents the simulation, while the procedures used to obtain it do not set out to mimic those of the real world. The output is thus a *simulacrum*, a representation of reality produced by the simulation. Interest focuses upon the relationship, the goodness of fit, between the simulacrum and the real data set which is being simulated.

An example of such a simulation is the XTENT model (chapter 3, above), by which the computer draws a political map, for instance of modern Europe, plotting in the notional boundaries, given only the location and size of the major cities, without information as to their territorial affiliation. The output is a map, the simulacrum which is to be compared with the real map (or, in archaeological cases, to be used as an approximation to a political map). But the procedures used to reach the conclusion do not follow the real course of events in time; this is not a dynamical model.

In the second class of model, it is not the final state, the terminal output at the last time point, which particularly interests us, but the sequence of events in time. The successive sequence of events experienced by the model is supposed to mirror those experienced in the real world: the real trajectory is *mimicked* by that of the model. Moreover the very computations undertaken in the course of the simulation correspond, at least notionally, to real activities. As Bell has said: 'Every transformation in the computer is also going on out there in the world.' Such is certainly the case for all System Dynamics models, including Zubrow's and Aldenderfer's ABSIM simulation (1981b).

It is arguable, therefore, that any simulation, to qualify as such, must produce as output a pattern representing the real world at a given point in time (that is, a simulacrum). Otherwise it must mimic the world by undergoing a series of operations, each of which has a real-world analog and by producing a series of values representing the trajectory through time of one or more state variables of the system.

Procedures which do not show one or the other (or both) of these features may well require a great elaboration of calculations to reach a final output value, but they are computations, not simulations.

During the discussion of Cordell's Wetherill Mesa analysis (Cordell 1975, 1981), the interesting methodological question was introduced as to whether the procedure used is, in the strict sense, a simulation. Low (1981) raised the point, suggesting that it might rather be seen as a procedure for *calculating* abandonments. The input is a time series, employing the dendroclimatic data of Fritts and others, and Cordell formulated rules by which the suitability for settlement of different areas of land in the study region could be assessed given the varying climatic data, hence allowing quantitative data for abandonment to be predicted. Sabloff, replying to Low, argued on the other hand that this should be regarded as simulation, since a real process is being replicated.

The discussion proved illuminating, focusing on the presence or absence of feedback within the model, feedback being seen by some as an essential ingredient of a simulation model. It was pointed out that if the past system-state has any bearing on the present system-state, then feedback is operating. Models which are completely input-driven need not have any feedback component. But a continuous trajectory – a feature of all living systems – is not possible without feedback.

The other notable feature of the Wetherill Mesa case is its extraordinary success rate. The predictions for inhabited sites with consecutive occupations, for successive phase transitions, compared with those observed in the field, were respectively 98.6, 96.6, 90.0 and 100.0 per cent. In this sense, it must surely be one of the most successful predictive models in the history of archaeology.

The same question of status, with respect to simulation, was raised in relation to some of Day's economic analyses (1981). In some cases, although successive system states could be completed graphically by iteration, the terminal state of the system was directly predicted by the analysis as much as by the iteration procedure. The question is perhaps ultimately a verbal one, and interest focused on the power of this approach to clarify issues and make predictions in a manner which raised no objections from the 'substantivist' anthropologists present.

In common with recent work by Reidhead (1976) and Keene (1979), Day's work appears to transcend the rather arid debate between 'formalist' and 'substantivist' economic anthropologists and may prove the forerunner to the general (and long overdue) application of formal economic models to early societies. The application advocated by Day of recursive programming – that is to say, successive optimization procedures employing feedback – offers a

very appropriate model both for the process of human learning and development at the individual level, and of the developing adjustment of a culture system to its environment. Recursive programming could clearly be used to simulate the developing subsistence and economic strategies of the community or culture against a background of environmental change, both directional and stochastic. Moreover, with its emphasis on cognitive aspects, on the process of 'learning' which a system undergoes as it adapts, the approach goes beyond rigidly deterministic models.

Three very different objectives were evident among the simulations discussed. The first, exemplified by Cordell's work, was the desire to obtain output data that would closely match the data collected in the field for the real case. The object of the simulation here is to offer a test to the underlying assumptions and theory, from which the algorithm for the simulation has been derived. As K.L. Cooke (1981) has concisely pointed out, the sequence of operation in such cases is always concept → theory → algorithm → simulation run → output → test.

It should be stressed that many simulations involve the use of a stochastic process. In this way, a rather elaborate body of data can be generated, using very simple underlying rules together with the stochastic input. This was not a feature of Cordell's model as expounded, but the procedure is seen in most models where spatial pattern is generated. At the end of the simulation, the output is usually compared with the real data.

Aldenderfer's Aboriginal Simulation (ABSIM) model (1981b) avowedly had a different objective. His ultimate aim was to generate data which could then be used to test taxonomic methods upon which he was working. Here is an unusual case where the output is to be used, in its own right, for further procedures, rather than to be tested against real data, as is more often the case.

For us, however, the ABSIM model had a different interest, and the taxonomic procedures for the investigation of which it was originally designed were neither presented nor discussed. Instead, interest focused upon a third use of simulation studies: simulation as a heuristic exercise. Here the desire to simulate the processes of lithic assemblage formation led to a very careful formal analysis of the factors involved, in a manner more explicit and precise than had hitherto been undertaken. There is no doubt that the discipline imposed by constructing a simulation procedure, such as can form the basis for a satisfactory algorithm, brings to light interesting hidden problems which remain hidden in less painstaking investi-

gations. As an aid to clear thought about processes – whether formation processes or culture processes of other kinds – the construction of a simulation has considerable heuristic value.

The fourth function of a simulation, discussed in the next section, is more open-ended than these, which are concerned chiefly (a) with testing the further implications of a number of initial assumptions; (b) with investigating these assumptions more deeply; or simply (c) with generating data for other purposes. The framework can, however, be considerably broader.

The Simulator as Demiurge

PRODUCER: And where's the script?

FATHER: It is in us, sir. (*The actors laugh.*) The drama is in us. We are the drama and we are impatient to act it – so fiercely does our inner passion urge us on.

Once the characters are alive . . . Once they are standing truly alive before their author . . . He does nothing but allow the words and gestures they suggest to him . . . And he must want them to be what they want themselves to be. When a character is born he immediately acquires such an independence . . . Even of his own author . . . That everyone can imagine him a whole host of situations in which his author never thought of placing him . . . They can even imagine his acquiring, sometimes, a significance that the author never thought of giving him.

Luigi Pirandello

Six Characters in Search of an Author (1954, 11, 58)

It is one thing to make certain assumptions, posit certain relationships, and construct a device that will follow these and explore their implications. That, as discussed in the preceding section, implies working within a well-defined frame of reference. It is quite another undertaking to seize upon certain ideas as component parts, to fit them together without any clear notion of exactly where they may lead, and to use the simulation as a means of exploration toward lands as yet uncharted. In this sense, the simulator is like an inventor, creating a device about whose behaviour and properties he is far from certain. He does not know where his brainchild may lead him: possibly in directions remote from his expectation.

Perhaps the most exciting moment in simulation modelling is when the machine is ready for lift-off. As the system proceeds through its dynamic trajectory, it can develop modes of behaviour which in Forrester's term are 'counter-intuitive' (Forrester 1973), and wander off, like the utensils bewitched by the Sorcerer's Apprentice or like Count Frankenstein's monster, into unexpected system states.

Zubrow's interesting work (1981) catches something of this spirit. He has employed Forrester's *Urban Dynamics* model without any very thoroughgoing scrutiny of the underlying theory or assumptions. Instead, he has taken considerable care that the equations for each component should be modified so as to be as appropriate as possible to ancient Rome, and he has gone to some trouble to provide suitable input data. Then it is a case of 'Chocks away, and clear for take-off.'

Zubrow terms his simulation a heuristic device, but I am not sure that this is the correct term. For the task here is not one simply of clarification or illumination or instruction. The exercise is more one of exploration than of elucidation, and the simulator is the inventor of an exploratory mechanism that goes ahead to fashion a simulacrum of the sensible world in the light of his ideas; Zubrow has (like Count Frankenstein) almost the status of demiurge.

The machine which he has created can, indeed, appear to have a life of its own, and thus it is not at all surprising that the simulator should find himself able to fashion and fit together certain components without being able to see where their interactions may lead them. Something of the same feeling of creating the components and then letting them get on with it was expressed by the playwright Pirandello in the construction of his comedy *Six Characters in Search of an Author*. The characters, created by him, had their own logic of behaviour, and the scenario was conceived as the playing out by these characters of their roles, with consequences which, although perhaps in a sense implicit within them, could not be or were not foreseen at the outset.

There is a genuine feeling of creation here. And the nub is this very quality of unexpectedness and unpredictability. For, as in any real and original act of creation, the qualities and potential of the product are not obvious or completely specified at the moment of formation; rather, they remain to be realized during the effective lifespan of the created object.

One point of great interest here is the methodological one. The simulator is not simply testing a hypothesis – in Zubrow's case, he avowedly was not taking explicit theoretical statements as his starting point. On the contrary, the procedure is to construct a system and see what happens. Yet this is not just building a device for its own sake: the exercise remains a simulation in that comparison with the real world is intended. The model becomes a successful one precisely when its behaviour in some respects resembles that of the real world.

In an interesting way, the usual systematic procedures, as set out in standard works on scientific method, are here reversed. In this case, the sequence is not to proceed from theory to deduction, to comparison with real data, to refutation or validation. When the fit between output (simulacrum) and real data is not a good one, the mismatch will not lead to automatic rejection of the generating model but rather to modifications in its structure, to produce a more closely comparable output. (It remains nonetheless true that if, after repeated improvements and modifications, the model is still not able to produce an output adequately resembling reality, it will probably be discarded.)

In many ways, here the activity is more akin to experiment ('Research and Development') than to the testing of pre-existing hypotheses. The model is being used as an exploratory device, and although there must still be underlying equations of state, they need not necessarily be exhaustively considered, nor even explicitly known, before the simulation is undertaken.

This view of the simulator in a creative role, rather than simply as a researcher using simulation in a highly controlled way to test the implications of pre-existing and carefully formulated hypotheses, may seem a shade imaginative. No doubt there is a risk of exaggerating the apparent autonomy in the behaviour of the simulation. The behaviour, it will be argued, is always implicit in the equations of state which govern the behaviour of the system which has been constructed, even if these equations have not been fully analysed at the outset.

Nonetheless, the methodological point made above and at the beginning of this chapter does have a certain validity. System dynamics modelling, and no doubt certain other simulation procedures, do make it possible for the model to be logically (and in reality) prior to the theory.

As an example, I should like to refer to a simulation exercise undertaken some years ago by Cooke and myself, with the assistance of Level (Cooke and Renfrew 1979; see chapter 11 above). The starting point was a systemic model concerned with the emergence and development of early Aegean civilization (Renfrew 1972). Subsystem interactions, involving seven subsystems, were summarized in matrix form. When all these interactions were positive, the simulation showed the system as a whole to grow and develop with increasing rapidity. When a number of these interactions were now given negative value (implying both negative feedback and some continuing depletions in the system), the system

in some cases, after a period of initial growth, suffered drastic collapse, in the manner to which we have become accustomed from Forrester's *Urban Dynamics* studies, and more particularly from the World Model popularized by the Club of Rome (Meadows *et al.* 1972).

Since the initial values which we were using, as well as most of the matrix coefficients, were highly hypothetical, we did not take this as an unduly adverse judgement on the viability of the Aegean culture system in the past or subsequently. Instead, we reduced the number of subsystems in order to reach some clearer understanding of what was causing the long-term instability of the system. It proved effectively impossible, using the model, to simulate a behaviour for the system which would have long-term stability. Subsequent formal analysis by Cooke showed that there were in fact only a very few equilibrium states for the system, other than those where the coefficients were either zero or infinitely large. In practice these were unstable states, since any small perturbation – which is inevitable as a result of 'noise' in a simulation procedure – results in a departure from the equilibrium and either a collapse or an 'explosion' (move toward infinite values) for the system. Oscillatory behaviour, however, is possible.

Here, then, is a case where behaviour unexpected by us emerged from the simulation. Further analysis was able to give explicit theoretical understanding of that behaviour.

Two further points of interest emerged here. In the first place, the experience led us to ask to what extent the unstable behaviour of the *Limits to Growth* model (Meadows *et al.* 1972) is a behaviour mode specific to that particular model – quite independent of the specific initial values used as input. If it should prove to have the inherently unstable behaviour shown analytically by Cooke to be a property of our own model, much of the discussion surrounding it, and certainly all the effort which has gone into the appropriate estimation of suitable initial values for the state variables, could be seen to be misplaced. The world may indeed be doomed to end, but the matter is not further illuminated by choosing for the world a model whose only terminal state is collapse. It follows that with any model it is important to test the full range of its behaviour modes, with a wide range of different estimates for the parameters involved, before imagining that it is simulating anything but its own behaviour.

The second point arises from this. At what stage is it appropriate to imagine such a model as simulating more than its own behaviour? The simulator is, of course, quite at liberty to give names to certain

variables in his equation: 'Let x_1 indicate population density, x_2 the rate of inflow of labour to the city', and so forth. But at precisely what point should these instructions be regarded as reasonable, and when should it be felt plausible that the model is making statements about the real world, or bearing on the real world? Zubrow has tellingly spoken of 'the willing suspension of disbelief.' When the model can produce so great a richness of behaviour that it is, in many ways, an effective simulation of the real world, there are now philosophical/methodological problems to be considered, indeed perhaps a new chapter to be written in the philosophy of science. The theatrical metaphor is not inappropriate, and there is an uncomfortable analogy between the simulator-demiurge and the dramaturge.

Long-Term Change: Evolutionary Machines?

The archaeologist has the opportunity to concern himself with long-term change, and hence with problems that only rarely confront many of those undertaking simulation in other disciplines. The consideration of long-term change, indeed, soon highlights the shortcomings of any explanatory model where the range of behaviour is in some ways predetermined at the outset. I have recently made this point elsewhere (Renfrew 1979, 37), and reproduce a table (table 1) intended to show that working *within* such a model is not enough; a way must be found to generate changes in structure. This would appear to be one of the current limitations of most dynamical systems modelling.

Table 1. The difficulty of modelling long-term change.

Approach	Short-term change	Long-term growth: Morphogenesis
Game theory	Different actual moves, different constraints	Different strategies, different utilities, different rules
System dynamics	Changes in value for flow and state variables, and for constraints in model equations of state	Changes in components of system, new feedback loops, new equations of state
Directed graphs	Transition to successive states	Radical changes in transition probabilities/times/costs. Possible addition of new states
Interaction matrix	Changing strengths of interactions between variables	Introduction of new variables

It is, however, a point addressed by Day in his discussion of meta-adaptation 'in which rules of behaviour or the population of agents, or both, are variables' (Day 1981). The problem is to explain structural changes taking place within the system that result in patterns of growth which are more than simply change in scale. Ideally we should like to see the model itself *generate* precisely these changes in its own structure. It is not enough simply to state thresholds beyond which different behaviour modes will take place, for it is inherent in the process of evolution, whether in the sphere of natural organisms or of human societies, that new, emergent properties are seen, that epigenesis (Waddington 1977, 110) takes place in the course of the unfolding through time, and that thresholds are defined, not externally, but by the developing system itself.

At this point, it is necessary to correct a serious misconception that substantially mars a previous Advanced Seminar volume (Hill 1977a). There, it is repeatedly asserted that the origin of changes must be sought *outside* the system in question. Thus Hill (1976b, 76) states:

No system can change in itself, change can only be instigated by outside sources. If a system is in equilibrium it will remain so unless inputs (or lack of inputs) from outside the system disturb the equilibrium. Of course, the individuals in a social system may consciously realize that change is necessary; but the reason the change is necessary lies in the relationship of the system with its environment.

Otherwise, why would change occur at all? In Buckley's approach, why would not the negotiation process eventually lead to stable equilibrium? Why do further 'tensions' arise? Where do they come from? Buckley claims that they are inherent in the system – but this is no answer; it begs the question.

The same point is made with great emphasis by Saxe (1977, 116): 'The processes that result in systemic change for all systems are and must be initiated by extra-systemic variables.'

Of course Hill and Saxe are correct that changes external to the system – and Saxe emphasizes rightly the effect of the arrival of Europeans on eighteenth- and nineteenth-century Hawaii – can be of determining significance, an important trigger to the developments which subsequently take place. But it cannot be too firmly stressed that change within a system can only be understood in terms of the internal structure of that system. Sometimes an exogenously determined alteration in certain variables may be seen as the 'cause' without which change would not have occurred. But in

other cases, no such striking environmental alteration is necessary.

Hill's discussion of 'equilibrium' here may be misleading. It is a matter for discussion whether any human cultural system is ever, from the standpoint of the long-term, in equilibrium; long-term processes of change are probably always taking place, whether or not they are of great significance for the future trajectory of the system. And the significance of such changes, often small in themselves, may not be easy to assess. Sanders, during discussion in this same volume, pointed out that population growth can be seen in some cases as a source of change internal to the system, but Saxe (1977, 289), in a reply which is incomprehensible to me, pronounced it external. A rather different view is presented by Perlman (1977) later in that volume, but nowhere is this unfortunate conclusion explicitly rebutted.

Archaeology has already faced this problem in recent years with the rejection of diffusionist explanations (Renfrew 1973; Binford 1968b). To define the system so that the source of change is always external to it is not merely to 'beg the question,' but to reduce all explanation to the status of a will o' the wisp, something which eludes our grasp. It 'has the effect of relegating to the wings all the action of the prehistoric drama' (Childe 1956, 154).

If the source of change is indeed seen as external to the system, it may be necessary to redefine the limits of the system so that the source can be included within it. In practice, however, what is ultimately of significance is not the possible origin of a change in an altered environmental variable, such as decline in rainfall. We seek to understand why and how this particular external change has long-term effects upon the structure (and hence upon the future trajectory) of the system, when other changes do not. Until we see this as the nub of the problem, the question has not been effectively posed at all.

This point has been well made in a recent paper by Bell and Senge (1980): 'A minimum standard for acceptance of many system dynamics models is generating the empirical behaviour of interest with *no exogenous time series inputs*.' Most real cases will indeed have such inputs, but to place the whole weight of the explanation upon them, as those cited above have sought to do, seems to defeat much of the object of the exercise.

The usefulness of systemic simulations for modelling long-term change is likely to depend upon their effectiveness for modelling changes in structure rather than simply changes in rate and level in an existing structure. And here it may be worth speculating whether

or not the approach may usefully be linked with others which have been employed in the discussion of morphogenesis. Here I am thinking, for instance, of Catastrophe Theory (Thom 1975; Renfrew 1978), where the focus is upon qualitative change, rather than on precise quantitative modelling.

The Catastrophe Theory approach encourages one to consider the global dynamics of the system and to think of the whole series of stable states which it may occupy. Some insight may thus be gained into its range of possible behaviours, without concerning ourselves at a detailed quantitative level with the values of all the variables in the system. This can bring out in a helpful way that, while the values of variables which we may consider external to the system (which will be numbered among the control variables) are of crucial relevance in determining at just what point certain changes may take place, the nature of those changes is governed by the global topology of the system itself.

Of course, the special strength of system dynamics modelling is that it allows one to work simultaneously at the detailed, quantitative level and to follow the broad, qualitative trends of the developing behaviour as it occurs. In some cases, however, other approaches may help one to see the forest for the trees.

This whole problem of the emergence of form is one which has fascinated mathematical biologists for many decades (Thompson 1942, Thom 1975, Waddington 1977), and I believe that their approach has much to offer us (Rosen 1979). To say that is not to overlook the numerous pitfalls in applying to cultural evolution some of the concepts of Darwinian evolution in a mechanistic way. Burnham (1973) has analysed the circularities that lurk in the application of the concept of 'adaptation' to human societies, and the Darwinian notion of monogenesis, which is of great value to the taxonomic paleontologists, has been positively misleading when applied to archaeology (Renfrew 1979). But there, in the workings of evolution, we can indeed view the genesis of structure, as it unfolds before our eyes. The models now being used to simulate this process will be of great relevance to us.

It is here that simulation may be of special value. If we can adequately simulate the structure and behaviour of an organism or organization, we should be able to see it survive or decline, grow or change along a series of different life histories or trajectories. What will happen if certain changes take place in the structure of the system, changes which we may regard as the analogue of genetic mutations? Under what circumstances along the time trajectory will

these minor 'mutations' bring about an evolutionary change of greater significance, a transformation to a new organizational form? In this sense we might be able to view our system simulations as evolutionary machines, plotting out for us different evolutionary paths, and exploring within a relatively short time in the laboratory possibilities which in reality would take centuries or millennia to realize.

In order to reach new structures, however, we must overcome the ultimately deterministic nature of most present system simulations. The system must be enabled to modify itself in ways both initially unforeseen and unpredictable. This implies, referring to table 1, that the system itself must be allowed to generate new feedback loops and new components in a way which is not completely determined at the outset. Perhaps this means introducing a stochastic element, on analogy with mutation. Sometimes that will lead to dysfunction, and the system may not survive. In some cases competition between coexisting systems will lead to some sort of Darwinian 'survival of the fittest,' but in others the system will have no competitor. Unfortunately, we have no obvious way of deciding now which possible 'mutation' is likely to occur and survive.

In some ways, there is little that is new in this notion of human societies and their operation being simulated by evolutionary machines. But in most existing simulations, the level of determinacy is so high that, given comparable initial conditions and similar external conditions along the trajectory, the final states are likely to be much the same. This is not the way to allow interesting new structures to emerge. We may, instead, find it fruitful to create system simulations which have the property of producing the unexpected.

Such a turn of events has intrigued many of those who have written of automata, from Mary Shelley to Arthur Clarke, and, in a different field, has disquieted those who are alert to the potentialities of recombinant DNA. It is perhaps time that, within the framework of our simulations, we should attempt to harness the unexpected in order to generate truly emergent properties. We are, however, dealing with human, not molecular, interactions, and the analogy with DNA must not be stretched too far.

These problems are usefully discussed within a rather different framework by Day (1981) under the heading 'Meta-adaptation.' In the discussion following the presentation of his paper, we began to consider some of the devices by which human societies have successfully countered disequilibrium. These are innovations, such as

the introduction of coinage, for instance, or of the limited liability company, or of currencies of account, which result in the long run in major changes in society which we can regard as structural changes.

One of the most promising features of the adaptive economics framework which Day proposes is the manner in which the development of 'disequilibrium mechanisms' may be approached. For it is very clear that what may originate as a mechanism simply to restore equilibrium may develop until it represents a significant structural change in the system, a major innovation in the unfolding development and growth process. This, then, is an aspect of the adaptive economics approach which will repay further exploration. Nor need the use of the term *economics* here inhibit the application of comparable ideas to other aspects of the culture system such as social structure. The underlying notion is a perfectly general one.

Some evolutionary changes in human societies, just as in living organisms, may be regarded as an increasingly effective *mapping* of the environment. Thus the development of the eye can, with the wisdom of hindsight, be seen as a positive adaptive response – a successful innovation in the mapping of an environment rich in electro-magnetic radiation of the appropriate wavelengths. An analogous case in human culture might be the tapping of atomic energy, or within the archaeological time range the 'invention' of metallurgy. Such inventions, which simply imply the new exploitation of a pre-existing (although not hitherto perceived) environment, may be regarded as an improvement in the mapping effected by the human society. It may be that, in cases where we as simulators already possess the relevant 'map,' an effective simulation of the mapping activity of past societies could be possible. (If we do not possess the relevant map – as in the case of controlled thermonuclear reactions – the simulation will not be so easy.)

These, however, are cases where the developments are ultimately determined by the structure of the environment, although in a different and perhaps more subtle sense than that of Hill and Saxe. More problematical, and, at the present stage in our understanding of human societies, more interesting, are the evolutionary changes within the structure of society, where the society is adjusting to essentially human problems. Many human social institutions are of this kind – all of the wide range of hierarchical structures, the various devices for controlling and processing information, the different types of exchange and the devices for facilitating it. When we have constructed simulations which can begin to come up with new structures of this kind – although not necessarily with just those

devices which societies known to us have produced, then we shall be at a new and more productive stage of modelling.

There are mere speculations at the moment. Yet many of the necessary ingredients are already available. It is an impressive experience to see how a skilled systems analyst can break a given problem down and express it in system dynamics terms. Low (1981) undertook this task for Cordell's Wetherill Mesa case, setting out in a logical analysis the loops and flows required to express fully the proposed insights into the causes of settlement abandonment. The next stage, which he could clearly have accomplished without difficulty, would be to write the necessary equations and proceed to a full simulation in the Forrester manner.

Already, then, it is possible to give an analytical account of the detailed behaviour of specified aspects of a culture system, which will include predictions about its future behaviour, at least over the short term. Such predictions are possible because we have an explicit understanding of the structure of the system, whose behaviour is thus, in a sense, determined, although it may not be known to us until the implications of the structure are explored by simulation.

The next stage must be to explore the future behaviour and the future structural evolution of systems whose evolution is not uniquely determined by their present form, but which have themselves the potential for generating emergent properties. [1981]

Note

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